Study of Insects," gives a short and popular account of entomology generally, by taking a series of types from amongst the best-known North American insects, and describing them in detail. We should have liked to find some of the descriptions rather more explicit, as they might have been, without any alteration in the size of the volume, if some of the illustrations had not been so frequently repeated. In a work like Euclid there is no doubt considerable advantage in having the figures so placed that it is not necessary to turn over the pages in referring to them, especially when it has to be read by boys; but when space is short and the subject of such general interest, we cannot help feeling that their repetition, three times in more than a single instance, is quite uncalled for. The author's own work at the development of Insecta, which he has published in the "Memoirs of the Peabody Academy of Science," enables him to take a larger view of his subject than that held by most. This is particularly indicated in the very suggestive chapter entitled "Hints on the Ancestry of Insects," in which the researches of Ganin, Lubbock, Brauer, Haeckel, and Müller are all brought to bear on such questions as the relation of the *Zoca* form of the embryonic Crustacean to the similarly undeveloped and generalised, here termed *Leptus*, form of Insecta, in which the configuration is ovate, the head is large, bearing from two to four pairs of mouth-organs resembling legs, and the thorax is merged with the abdomen; this general embryonic form characterising the larvæ of the Arachnida, the Myriapods, and the true Insects. The elaborate observations of the first-named of these authors on the development of *Platygaster error*, an ichneumon parasite, in the author's mind tend to confirm the theory held by him that the ancestry of all the Insects, including the Arachnids and Myriapods, should be traced directly to the worms. We recommend this small book to all interested in the progress of this branch of invertebrate zoology.

The Transactions of the Academy of Science of St. Louis, vol. iii. No. 1. (St. Louis, U.S., 1873.)

THIS volume contains a journal of the proceedings of the Society from March 1868 to January 1873, and a few papers *in extenso*. The latter are:—Notes on the Genus *Yucca*, by G. Englemann; On the new Genus in the Lepidopterous Family Tineidæ, with Remarks on the Fertilisation of the *Yucca*; and Supplementary Notes on *Pronuba yuccasella*, by C. V. Riley; Descriptions of North American *Hymenoptera*, by B. D. Walsh; Atmospheric Electricity, by Dr. A. Wislizenus, being the yearly report of atmospheric electricity, temperature, and humidity, from observations made at St. Louis; Catalogues of Earthquakes for 1871, by R. Hayes; and On the Occurrence of Iron Ores in Missouri, by J. R. Gage. Mr. Hayes, on the basis of the recorded earthquakes from 1739 to 1842 has found that the "largest maxima occurred in the years of the heliocentric conjunction and opposition of Jupiter and Saturn, with but three exceptions, and in these cases the increase began in those years, but the maximum was not reached till the following year." He suggests that "these planets induce electric currents which *call into action* those forces to which the causes of seismic phenomena are usually ascribed."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Herbert Spencer and *à priori* Axioms

MR. HERBERT SPENCER (vol. ix. p. 461) has "ended what he has to say on the vexed question of the origin of physical axioms" by laying down—

(1) That "the perceptions and inferences of the physicist cannot stand without *preconceptions which are the products of simpler experiences than those yielded by consciously-made experiments.*"

(2) That "the preconception which immediately concerns us is the exact quantitative relation between cause and effect."

(3) That "if definite quantitative relations between causes and effects be assumed *à priori*, the Second Law of Motion is an immediate corollary."

By speaking of it as an "*immediate corollary*," I presume that Mr. Spencer means that Newton's Second Law of Motion is the proposition obtained by substituting for the general term, *cause*, the particular term, *force*, and for the general term, *effect*, the particular term, *motion generated;* so that, according to Mr. Spencer, this law simply asserts "a definite quantitative relation between a force and the motion generated by that force." But surely the quantitative relation asserted by Newton is not only *definite*, but is further the *special* relation of *proportionality;* so that, if the law is an immediate corollary of an *à priori* assumption, the assumption must be that "the exact quantitative relation between cause and effect is that of direct proportionality," or in more familiar words, that "effects are proportional to their causes." Perhaps this is what Mr. Spencer meant to assert. At any rate let us admit it as a definite basis for reasoning, and endeavour to deduce some consequences from it.

"The cause of a stone falling when left to itself is its weight; but 'the greater the cause, the greater the effect,' therefore the greater the weight of the stone the more quickly will it fall, and thus of two stones let fall from the same height, the heavier will reach the ground sooner than the other." Something of this kind, it may be presumed, was the argument of Aristotle and his followers before the age of Galileo: and how on *à priori* principles is it to be refuted? Of course it is disposed of at once by the simple observation that the same force does not produce the same motion in different masses: but independently of some such observation or experiment, it seems to me impossible to deny that it *may be* true, though even an *à priori* philosopher might show that, as other alternatives are conceivable, it is not *necessarily* true. As a matter of historical fact, Galileo refuted it once for all by the "consciously-made experiment" of letting two different weights fall simultaneously from the leaning tower of Pisa.

But it may be said that the above argument is hardly "*definitely* quantitative." Let us then examine Newton's Second Law of Motion as an "immediate corollary" of our *à priori* assumption. Here the cause is "the motive force impressed," and the effect "the alteration of motion." But then the question arises—how are the quantities of this cause and effect to be measured? Newton carefully defines quantity of motion as proportional to *mass* and *velocity* jointly; that is, he measures it by *momentum*. From another point of view it would have been correct to measure quantity of motion by *kinetic energy* or *vis viva*, that is, as proportional to mass and the *square* of the velocity jointly. Further the "alteration of motion" might be measured either with respect to a given time or to a given space. Newton implies the former, and consequently the explicit statement of his second law is that "the *momentum* generated *in a given time* by an impressed force is proportional to that force." Substitute for this "the momentum generated in moving through a *given space*," or "the *kinetic energy* generated in a given time," and the law becomes untrue. Substitute "the *kinetic energy* generated in moving through a *given space*," and we have a law which is true, but not that which Newton asserted as his second law. Now among these four alternatives how is our *à priori* philosopher to decide? He might perhaps analyse them further and show that some of them are inconsistent with the others, and I believe he might reduce the questions to be decided to still simpler ones; but I fail to see (in common, I believe, with everyone who has thoroughly grasped the fundamental principles of rational mechanics) how, without recourse to consciously-made observations or experiments, he could arrive at a certain conclusion.

May we not say then that these great *à priori* principles, whatever value they may have in a "System of Philosophy," are of little avail in any special science, and that the "axioms" of such science, however much they may involve these principles, are not mere "immediate corollaries" therefrom?

If not intruding too much on your space, I am tempted to apply to Mr. Spencer's great principle of the "Persistence of Force" the same mode of treatment as I have applied above to the principle that "effects are proportional to their causes."

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Mr. Spencer distinctly refuses to identify this principle with the great physical principle of the Conservation or the Persistence of Energy, the firm establishment of which undoubtedly marks one of the most important epochs in the history of Science. Force, in Mr. Spencer's use of the term, includes numerous species of which energy is but one. I feel sure that every mathematician and physicist would protest against the inclusion under one term of magnitudes of such different kinds as statical force and energy, or the work done by such a force; but not to dwell on this, I believe that Mr. Spencer would certainly acknowledge as one of his species, that which (in my view) is alone properly termed Force, namely, such as can be measured in terms of the weight of a pound or a gramme. What then does Persistence of Force of this kind mean? Does it mean that the numerical sum of the intensities of all the Actions and Reactions throughout the universe is constant? If so, it is untrue: for, to take a simple illustration, if a weight be supported, first by a single string, and then by two strings not vertical, the tensions are quite different in the two cases, and there is no equivalence between those which disappear, and those which are introduced in passing from one to the other. If not, we must take account of the *directions* of our forces, and then, if it mean anything, it appears to be but the expression of Newton's Third Law that "action and reaction are equal and contrary" in this form:—"The algebraical sum of all the forces throughout the universe is persistently zero." To every mathematician, at any rate, this assertion and the assertion that "the sum of the energies of all kinds throughout the universe remains persistently of the same definite numerical amount" are assertions of facts of such different orders, that to class them together is rather to introduce confusion of thought than to establish a grand general principle.

I have offered the above remarks because it appears to me only fair to the author of the article on Herbert Spencer in the *British Quarterly Review* to show that it is felt by others, who have made a study of the fundamental principles of rational mechanics, that his strictures on Mr. Spencer's treatment of those principles are in all essential points fully justified, however much they may wish that the expression of those strictures had been in some instances modified in its tone.

The Park, Harrow, April 20 ROBERT B. HAYWARD

I THINK it is positively due, not only to the writer of the now famous article in the *British Quarterly Review*, but to Newton's memory and to Science itself, that the correspondence which has been going on should not seem to terminate as a drawn game, at any rate in the opinion of some bystanders, who may from their antecedents be presumed competent to judge.

That Mr. Spencer will ever be convinced is, I suppose, hopeless; I at any rate am not going to try to convince him. But I can assure the *British Quarterly Reviewer* that he has my very deepest sympathy in his argument with an antagonist who is at once so able a master of fence as Mr. Spencer, and yet is so intensely unmathematical, it would seem, as to pass from "exact quantitative relation" to "proportionality;" or as to talk of the effect of a force, without defining how the effect is to be measured, without feeling the slightest difficulty.

Nor does it seem that Mr. Frankland, in NATURE, vol. ix., p. 484, is quite justified in his conclusion that the truth lies *between* the two opposite views. And his own view is in fact entirely coincident with the Reviewer's, except, perhaps, on a point which is not relevant to the controversy, viz. how far the experimental proof of the so-called physical axioms is *complete*.

Will it comfort the Reviewer if I tell him some of my own experience? I, too, read Spencer after my degree; and on the first reading of the "First Principles" came to the sad conclusion that I had not understood any mathematics properly; so much fresh light seemed to be thrown on them. I read it again, and more critically, and doubted whether Spencer was quite correct. I read it again, and concluded that he was wrong in his physics and mathematics. I ought to add that I too was, like the Reviewer, A SENIOR WRANGLER

I AGREE so fully with the chief contents of Mr. Frankland's letter (vol. ix. p. 484), that I wish to call his attention to one point in which his letter seems to me calculated to mislead.

He says, "the pure empiricists argue that because certain observed results coincide with the results of calculation, therefore the assumptions on which the calculation was based must be true. Now without doubt the *demonstrative* character of this inference vanishes entirely under Mr. Spencer's searching criticism. But it seems to me that a *high probability* remains."

Now, in the name of pure empiricists, I must protest against our being supposed to think that anything "must be true" in any other sense than that there is a "high probability" of its truth. I cannot refer to a better exponent of our views on this point than Prof. Clifford, to whom Mr. Frankland himself refers. And the idea of our having to thank Mr. Spencer for showing that the inductive proofs of the laws of motion (or of any other physical truths) are not *demonstrative* in any other sense than the above is quite new to us. What Mr. Spencer has done is to bring up instances of this so-called imperfectness in the demonstration as evidences that no *à posteriori* proof of the proposition can exist, when in point of fact they are specially characteristic of such a proof.

Those of your readers who have examined Mr. Spencer's ingenious proof of the second law of motion, contained in his last letter to NATURE (vol. ix. p. 461), will not ascribe my not immediately answering his letter to any difficulty in so doing.

THE AUTHOR OF THE ARTICLE IN THE BRITISH QUARTERLY REVIEW

Lakes with two Outfalls

IN NATURE, vol. ix., p. 485, Mr. Craig Christie begins a letter "to correct a mistake as to a matter of fact:" "Loch-na-Davie, Arran, has *two* outlets, as is correctly represented in the Ordnance map;" and he ends his letter: "I think Colonel Greenwood ought at least to have made himself acquainted with the Ordnance map."

I take the liberty to enclose to you the new Inch Ordnance map of Arran, to which my letter in vol. ix. p. 441 referred. You will see that as "a matter of fact" the map does *not* give two outlets, but only one.

I need not ask for your valuable space in reference to Mr. Christie's own "matters of fact," since my views with reference to them are printed in the *Athenæum* of July 22, 1865. He will see there that I have not only "walked up the north stream from Loch Ranza," but also by Glen Catacol and Glen Dzeven, and a third time from Corrie by Glen Sannox over the water-parting. Also that I have sounded the whole of this little pool of bog-water by walking it, bare-legged, without being over my knees in the deepest part, which was at the south end, where the only outlet is to Glen Iorsa.

I shall have the pleasure to communicate with Mr. Thelwall in reference to his obliging letter.

GEORGE GREENWOOD

[The Ordnance map forwarded to us by Colonel Greenwood gives only one outlet to Loch-na-Davie.—ED.]

As this subject appears to me to possess an interest apart from the issues hitherto in question, I trust you will allow me a little of your space.

From the fact that lakes do not ordinarily occupy the crest of a watershed, it would *à priori* appear more likely that a double outfall, if it exist, should lie in or towards adjacent districts than connected with opposed valley systems. The following instance, which I observed in Norway last summer, is, in view of Colonel Greenwood's letter (NATURE, vol. ix. p. 441), worth mentioning. The lake exhibiting it lies about two miles inland (N.W.) from the elevated coast which faces Trondhjem, and is named Stor Lake; its length—nearly parallel to the Trondhjem fjord—is about seven miles, its greatest breadth about two. Like many Norwegian lakes, it presents a *facies* different to what we are most familiar with in Britain. Instead of occupying a single valley-basin, it consists of a chain of minor basins strung along an axis of depression (probably a pre-existing valley), and each separated from its neighbours by the subsided walls of the valley of which it is the cup-like enlargement. The form of Stor Lake is irregular, with long arms or creeks extended (obliquely to its longer axis) into the mouths of the valleys. In such lakes it might be expected now and then that the effluent waters should pass out at more than one of these channels, and in Stor Lake such is the case. One stream is discharged from one of the component basins, nearly at right angles to the lake's greatest length, the other issues along the depression on which I have said the basins are "strung"—bead-like. The former opening is of post-glacial date, and is superseding the original one for several reasons:—(1) it flows along the strike of a homogeneous bed of schist, whereas the other cuts across beds of various textures, and (2) its volume is greater. Its rival bears evident traces of

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